unless the number is very large. In the latter event, representative opinions, with names, could be published.

Whether a plan of this sort could be extended to include the scientific workers of the world (or such of them as might be concerned with the particular matter under discussion) is a difficult question. Efforts of various kinds are being made at the present time to bring the scientific men of the world into closer touch with one another, and it is perhaps not quixotic to suppose that eventually they will be at least as ready and as competent to act together as are those of America at the present time.

T. D. A. Cockerell
University of Colorado,
March 28

THE FUTURE OF NOMENCLATURE

Professor T. D. A. Cockerell's discussion under the heading "Genera without Species" recently published in Science,[1] is of great pertinency at the present time.

Without discussing the question concerning the validity or non-validity of genera described without species named in connection with them, or genera proposed with undescribed types, a question familiar to every systematist, and one which I hope to see discussed by others more competent and learned than myself, I desire merely to make one or two general observations concerning nomenclature as a whole, its function and its future.

Before doing this, however, some remarks concerning the cases considered by Professor Cockerell may not be out of place or without some use.

In the first class of cases, a genus described in the past without a species named in connection with it, I consider as being non-existent—a *nomen nudum*—and it remains such unless subsequently its author or some other refers to it a properly described type species. The genus being non-existent, its name does not have to be recognized again as being that of a zoological unit, excepting as a matter of wisdom; if used again, it has no status unless used as a name based on some definite type species designated subsequent to its original description, and in such cases the original author of the name should be held responsible for it, mainly as a matter of clearness. I agree with Professor Cockerell's interpretation of the code in cases of this kind. Whether or not these *nomina nuda* made in the past should be used again depends largely on circumstances, and is almost a matter of individual judgment. I think they should be used in most cases to prevent questions of obscure homonymy, and confusion arising from other sources. These remarks bring me to the point I had in mind in regard to this class of cases. I have referred to them as occurring in the past. Should they be allowed to occur in the future? There is no excuse at the present time for cases of the kind being made, but some provision should assuredly be made in the code to prevent them. The code should state that after such a date (1900 recommended) genera proposed or described without species named in connection with them should be considered as being without status in nomenclature and ignored accordingly, as newspaper descriptions are ignored. If this is possible, the systematists of the present and future will not have constantly accruing cases of the kind to deal with, or be in danger of their common occurrence, and the old cases would be gradually cleared up.

As to the second class of cases. We may not know, or attempt to define, the exact differences between a species and a genus in monotypical genera; still we do know as a matter of experience that when an author briefly defines a new genus in a diagnostic table of genera of a group and merely mentions a species as type, without describing it and yet follows the rules of binary nomenclature, he has not done all that is necessary to make it recognizable. As a matter of fact, we know that he has not described the species by diagnosing the genus, for the simple reason that the species can not be recognized. As a case in point: In a group of insect parasites of the Hymenoptera, the late Dr. Ashmead, in a table of the genera of a tribe of the Sphegigasterinæ describes or defines a new genus called *Pachycrepoideus,* merely naming a type

[1] N. S., XXIX., February 26, 1909, pp. 339–340.

species in parentheses, without describing it. As defined, the genus was based on but one or two general diagnostic characters. Just recently, I had occasion to deal with the genus in connection with certain parasites of the common house-fly (*Musca domestica* Linnæus); the species of parasite agreed with the brief description of the genus in every particular, as far as it went. The question arose, was it the type species. To decide from the description of the genus was nothing less than pure guesswork. *The generic description or diagnosis did not characterize the species.* Fortunately, I knew that the type species was in the National Museum at Washington and through the kindness of the authorities of that institution I learned that the species which I had under consideration was not only not the type species of *Pachycrepoideus*—a fact which was not strange—but that it differed so much from the genus itself, in other structural, generic characters not mentioned in the description, that it formed a distinct genus in the same division of the tribe.[2] This case is but one in a number of similar ones which occur in the parasitic Hymenoptera. To say that the type species of this genus is described is to say, it seems to me, that naming a thing describes it. To hold such a view is to recommend the practise of naming instead of describing, which is practically what the case just cited amounts to. It is merely another way of saying that to generalize on the whole, is describing and making recognizable all of the component parts. As genera are being described at the present day, even, the practise is a most dangerous one, for unless the type species are soon described they become unrecognizable, the genera practically come under the first class of cases considered, and progress demands a removal of the obstructions and they fall or have to be reconstructed. Future cases of this kind should be prevented.

The main point to which I wish to call attention, however, is not what to do in past cases of the kind considered, but what to do in order to prevent their occurrence in the future. In this matter, we must leave the past behind us and build for the future. Looking ahead, not behind, the whole question of nomenclature turns on a single point, that of identity. If those who come after are to build upon what we are doing in the present, stability in nomenclature will primarily depend on the means we leave behind us for the identification of the things we name. Nomenclature, as we all know, is but the tool, the means to the end of systematic work; the end is fundamentally concerned with identity, identity based on, and dependent upon, definition or description. In the great present-day activity in systematic work, enlightened by past failures and errors, it seems to me to be a deliberate fault for a systematist to cause cases like the kind we have considered. We must all know that what in the far past was regarded as a genus, to-day has become a family or other higher group, and in all probability what is regarded as a genus to-day, in the future may become a much higher group. We have all learned, ere this, that the greatest causes of error, delay and obstruction to progress in systematic zoology at the present time is the meagerness of definition or description of genera and species. Of what use is it to us, to the future, to the race at large, to science, for a systematist, although a recognized authority in the group in which he is working, to describe briefly, unrecognizably, new genera or new species? Does the fact that we know through his efforts that they exist help any of us, help science? Assuredly not; if the units are not recognizable they are nothing more or less than obstructions. Fifty years hence, the systematists will be considering them under the same general classes of cases that we at the present are considering the poorly described genera and species of fifty or more years ago. Having what will then be considered but one or two very general diagnostic characters upon which to base conclusions, in all probability they will be at an utter loss to know into what families or other groups to place them. Of what possible benefit is it, therefore, to describe these things unless we use in every case all the means at our command to make them

[2] I have since learned that my species was the type species of *Pachycrepoideus*. This does not alter the case.

recognizable; will they not in the end become mere *nomina nuda?*

Identity being the fundamental basis of nomenclature, and intimately connected with the end of systematic work itself, it seems utterly absurd to ignore it or to give it but passing attention. Therefore immediate steps should be taken to insure it. Instead of having an international code of nomenclature *recommended* to zoologists, to be followed at their discretion, we have advanced far enough to have one which should be *enforced* by legislation of some such body as the International Zoological Congress, no systematist being recognized unless adhering rigidly to its rulings. At first thought this step may appear to be visionary, as we can not by law control such intangible or incorporeal things as the individual judgments of men concerning what is or is not a good description of a thing; nevertheless, we can prescribe, in cases of the kind considered, what shall or shall not be done in the future. Genera described without species can be rigidly barred; genera described without a description of the type species upon which they are based can be treated likewise. The authors of such genera could be reprimanded or discountenanced, in a sense proscribed. Further a date of departure for a new system of nomenclature based on the future should be designated, for the questions of the past should be studiously avoided in the future, and the new code should be conceived in the spirit of the future, that is to say, in the spirit of expansion, of progress. Such a code, for instance, could provide for the future cases coming under article 21 of the international code, which should be framed along lines tending to make descriptions infinite in detail. For example, an *indication* should not be allowed to hold for present-day or future descriptions and some provision should be made for the compulsory deposition of types in accredited museums. I have mentioned but one or two points which such a code should be expected to cover; for its development and adoption I can hope only; for these few suggestions, I beg the consideration due to the spirit in which they are offered.

The end should always be in mind; we must broaden our view-point; let us look to the future, for properly the present belongs to it.

A. Arsène Girault
University of Illinois,
March 1, 1909

Scientific Books

The Origin of the Vertebrata. By Walter Holbrook Gaskell. Longmans, Green & Co. 1908.

Professor Gaskell during the past two decades has published an extended series of papers which have aimed to convert morphologists to the view that vertebrates are descended from arachnids. These papers, with additions and corrections, are now brought together in volume form. We suggest, however, the book's title "The Origin of the Vertebrata" is chosen inaptly. It should have read "The Supposed Arachnid Origin of the Vertebrata," or, better, "A Plea for the Rejected Theory of the Origin of the Vertebrates from Arachnids." For it is hardly fair that the purchaser of this book should believe that he has here a résumé of our knowledge of the ancestry of the vertebrates. He is given merely a one-sided view of the whole intricate problem.

It is just to say that Gaskell has devoted himself generously to the task which he has sought to accomplish. His work shows that he has been earnest and tireless, that his reading has covered a field much wider than that of the usual promoter of a lost cause—that he is not one of those whose effort is measured in terms of success, for he would himself admit that even his friends (and he has many sympathetic ones) in the wide zoological fraternity, do not subscribe (there is scarcely an exception) to a single tenet of his heretical morphology. If he had been trained as a morphologist instead of as a physiologist. perhaps he himself would never have developed his theory.

There has been of late years a tendency to ignore Gaskell's writings on the ground that his arguments, having been weighed carefully, have been found wanting. Then, too, we have lost zest for discussing his difficult theses, *e. g.*, that the arachnid gut and nervous cord fused